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ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. II

Such, then, are the facts, which call for an interpretation. More than one has been proposed; but it will be well, before discussing them, to arrive at some idea of the climate of these islands during the colder part of the Glacial epoch. Unless that were associated with very great changes in the distribution of sea and land in northern and northwestern Europe, we may assume that neither the relative position of the isotherms nor the distribution of precipitation would be very materially altered. A general fall of temperature in the northern hemisphere might so weaken the warmer ocean current from the southwest that our coasts might be approached by a cold one from the opposite direction.[35] But though these changes might diminish the difference between the temperatures of London and Leipzig, they would not make the former colder than the latter. At the present day the snow-line in the Alps on either side of the Upper Rhone Valley is not far from 8,000 feet above sea-level, and this corresponds with a temperature of about 30°. Glaciers, however, are not generally formed till about 1,000 feet higher, where the temperature is approximately 27°. Penck and Brückner place this line during the coldest part of the Ice Age at about 4,000 feet.[36]

[35] Facts relating to this subject will be found in "Climate and Time," by J. Croll, ch. II. and III., 1875. Of course the air currents would also be affected, and perhaps diminish precipitation as the latitude increased.

[36] *Loc. cit.*, p. 586, et seq. They say the snow-line, which would mean that the temperature was only 12° lower than now; but as possibly this line

In that case the temperature of the Swiss lowland would be some 15° lower than now, or near the freezing point.[37] If this fall were general, it would bring back the small glaciers on the Gran Sasso d'Italia and Monte Rotondo in Corsica; perhaps also among the higher parts of the Vosges and Schwarzwald.[38] In our own country it would give a temperature of about 35° at Carnarvon and 23° on the top of Snowdon, of 32° at Fort William and 17.5° on the top of Ben Nevis. If, in addition to this, the land were 600 feet higher than now (as it probably was, at any rate in the beginning of the glacial epoch), there would be a further drop of 2°, so that glaciers would form in the corries of Snowdon, and the region round Ben Nevis might resemble the Oetzthal Alps at the present day. This change of itself would be insufficient, and any larger drop in the ocean level would have to be continental in its effects, since we can not assume a local upheaval of much more than the above amount without seriously interfering with the river system of north central Europe. But these changes, especially the former, might indirectly diminish the abnormal warmth of winter on our northwestern coasts.[39] It is difficult to estimate the effect of this. If it did no more than place Carnarvon on the isotherm of Berlin (now lower by 2°), that would hardly bring a glacier from the Snowdonian region down to the sea. At the present time London is about 18° warmer than a place in the same latitude near the Labrador coast or the mouth of the Amur River, but the removal of that difference would involve greater changes in the distribution of sea and land than seems possible at an epoch comparatively speaking so recent. I am doubtful whether we can attribute to changed currents a reduction in British temperatures of so much as 11°; but, if we did, this would amount to 28° from all causes, and give a temperature of 20° to 22° at sea-level in England, during the coldest part of the glacial epoch.[40] That is now found, roughly speaking, in Spitzbergen, which, since its mountains rise to much the same height, should give us a general idea of the condition of Britain in the olden time.

What would then be the state of Scandinavia? Its present temperature ranges on the west coast from about 45° in the south to 35° in the north.[41] But this region must now be very much, possibly 1,800 feet, lower than it was in preglacial, perhaps also in part of glacial, times.[42] If we added 5° for this to the original 15°, and allowed so much as 18° for the diversion of the warm current, the temperature of Scandinavia would range from 7° to — 3°, approximately that of Greenland northwards

might then more nearly correspond with that of glacier formation, I will provisionally accept the higher figures, especially since Corsica, the Apennines, and some other localities in Europe, seem to require a reduction of rather more than 12°.

[37] It would be 32.5° at Zurich, 31.6° at Bern, 34.1° at Geneva, about 39.0° on the plain of Piedmont and 36.0° at Lyons.

[38] See for particulars the author's "Ice Work" ("International Scientific Series"), p. 237.

[39] For much valuable information on these questions see a paper on the Climate of the Pleistocene Epoch (F. W. Harmer, *Quart. Journ. Geol. Soc.*, LVII., 1901, p. 405).

[40] The present temperature in Ireland over the zone (from south of Belfast to north of Galway Bay) which is supposed to have formed the divide of the central snowfield may be given as from 49° to 50°, nearly the same as at the sea-level in Carnarvonshire. Thus, though the district is less mountainous than Wales, it would not need a greater reduction, for the snowfall would probably be rather larger. But this reduction could hardly be less than 20°, for the glaciers would have to form nearly at the present sea-level.

[41] It is 44.42° at Bergen, 38.48° at Bodo, 35.42° at Hammerfest, 41.36° at Christiania and Stockholm.

[42] For particulars see *Geol. Mag.*, 1899, p. 97 (W. H. Hudleston) and p. 282 (T. G. Bonney).

from Upernivik. But since the difference at the present day between Cape Farewell and Christiania (the one in an abnormally cold region, the other in one correspondingly warm) is only 7°, that allowance seems much too large, while without it Scandinavia would correspond in temperature with some part of that country from south of Upernivik to north of Frederikshaab.[43] But if Christiania were not colder than Jakobshavn is now, or Britain than Spitzbergen, we are precluded from comparisons with the coasts of Baffin Bay or Victoria Land.

Thus the ice-sheet from Scandinavia would probably be much greater than those generated in Britain. It would, however, find an obstacle to progress westwards, which can not be ignored. If the bed of the North Sea became dry land, owing to a general rise of 600 feet, that would still be separated from Norway by a deep channel, extending from the Christiania Fjord round the coast northward. Even then this would be everywhere more than another 600 feet deep, and almost as wide as the Strait of Dover.[44] The ice must cross this and afterwards be forced for more than 300 miles up a slope, which, though gentle, would be in vertical height at least 600 feet. The task, if accomplished by thrust from behind, would be a heavy one, and, so far as I know, without a parallel at the present day; if the viscosity of the ice enabled it to flow, as has lately been urged,[45] we must be cautious in appealing to the great Antarctic barrier, because we now learn that more than half of it is only consolidated snow.[46] Moreover, if the ice floated across that channel, the thickness of the boulder-bearing layers would be diminished by melting (as in Ross's Barrier), and the more viscous the material, the greater the tendency for these to be left behind by the overflow of the cleaner upper layers. If, however, the whole region became dry land, the Scandinavian glaciers would descend into a broad valley, considerably more than 1,200 feet deep, which would afford them an easy path to the Arctic Ocean, so that only a lateral overflow, inconsiderable in volume, could spread itself over the western plateau.[47] An attempt to escape this difficulty has been made by assuming the existence of an independent center of distribution for ice and boulders near the middle of the North Sea bed[48] (which would demand rather exceptional conditions of temperature and precipitation); but in such case either the Scandinavian ice would be fended off from England, or the boulders, prior to its advance, must have been dropped by floating ice on the neighboring sea-floor.

If, then, our own country were but little better than Spitzbergen as a producer of ice, and Scandinavia only surpassed southern Greenland in having a rather heavier snowfall, what interpretation may we give to the glacial phenomena of Britain? Three have been proposed. One asserts that throughout the glacial epoch the British Isles generally stood at a higher level, so that the ice which almost buried them flowed out on to the beds of the North and Irish Seas. The boulder clays represent its

[43] Christiania and Cape Farewell (Greenland) are nearly on the same latitude.

[44] For details see *Geol. Mag.*, 1899, pp. 97 and 282.

[45] H. M. Deeley, *Geol. Mag.*, 1909, p. 239.

[46] E. Shackleton, "The Heart of the Antarctic," II., 277.

[47] It has indeed been affirmed (Brögger, "Om de senglaciale og postglaciale nivaforandringer i Kristianiafelted, p. 682) that at the time of the great ice-sheet of Europe the sea-bottom must have been uplifted at least 8,500 feet higher than at present. This may be a ready explanation of the occurrence of certain dead shells in deep water, but, unless extremely local, it would revolutionize the drainage system of central Europe.

[48] *Geol. Mag.*, 1901, pp. 142, 187, 284, 332.

moraines. The stratified sands and gravels were deposited in lakes formed by the rivers which were dammed up by ice-sheets.[49] A second interpretation recognizes the presence of glaciers in the mountain regions, but maintains that the land, at the outset rather above its present level, gradually sank beneath the sea, till the depth of water over the eastern coast of England was fully 500 feet, and over the western nearly 1,400 feet, from which depression it slowly recovered. By any such submergence Great Britain and Ireland would be broken up into a cluster of hilly islands, between which the tide from an extended Atlantic would sweep eastwards twice a day, its currents running strong through the narrower sounds, while movements in the reverse direction at the ebb would be much less vigorous. The third interpretation, in some respects intermediate, was first advanced by the late Professor Carvill Lewis, who held that the peculiar boulder clays and associated sands (such as those of East Anglia), which, as was then thought, were not found more than about 450 feet above the present sea-level, had been deposited in a great fresh-water lake, held up by the ice-sheets already mentioned and by an isthmus, which at that time occupied the place of the Strait of Dover. Thus, these deposits, though indirectly due to land-ice, were actually fluviatile or lacustrine. But this interpretation need not detain us, though the former existence of such lakes is still maintained, on a small scale in Britain, on a much larger one in North America, because, as was pointed out when it was first advanced, it fails to explain the numerous erratic blocks and shell-bearing sands which occur far above the margin of the hypothetical lake.

Each of the other two hypotheses involves grave difficulties. That of great confluent ice-sheets creeping over the British lowlands demands, as has been intimated, climatal conditions which are scarcely possible, and makes it hard to explain the sands and gravels, sometimes with regular alternate bedding, but more generally indicative of strong current action, which occur at various elevations to over 1,300 feet above sea-level, and seem too widespread to have been formed either beneath an ice-sheet or in lakes held up by one; for the latter, if of any size, would speedily check the velocity of influent streams. Also the mixture and crossing of boulders, which we have described, are inexplicable without the most extraordinary oscillations in the size of the contributing glaciers. To suppose that the Scandinavian ice reached to Bedfordshire and Herts and then retired in favor of north British glaciers, or vice versa, assumes an amount of variation which, so far as I am aware, is without a parallel elsewhere. So also the mixture of boulders from south Scotland, the Lake District and north Wales which lie, especially in parts of Staffordshire and Shropshire, as if dropped upon the surface, far exceeds what may reasonably be attributed to variations amplified by lateral spreading of mountain glaciers on reaching a lowland, while the frequent presence of shells in the drifts, dozens of miles away from the present coast, implies a rather improbable scooping up of the sea-bed without much injury to such fragile objects. The ice also must have been curiously inconstant in its operations. It is supposed in one place to have glided gently over its bed, in another to have gripped and torn out huge

[49] See Warren Upham, "Monogr. U. S. Geol. Survey," XXV., 1896. This explanation commends itself to the majority of British geologists as an explanation of the noted parallel roads of Glenroy, but it is premature to speak of it as "conclusively shown" (*Quart. Journ. Geol. Soc.*, LVIII., 1902, 473) until a fundamental difficulty which it presents has been discussed and removed.

masses of rock.[50] Both actions may be possible in a mountain region, but it is very difficult to understand how they could occur in a lowland or plain. Besides this we can only account for some singular aberrations of boulders, such as Shap granite well above Grosmont in Eskdale, or the Scandinavian rhomb-porphyry above Lockwood,[51] near Huddersfield, by assuming a flexibility in the lobes of an ice-sheet which it is hard to match at the present time. Again, the boulder clay of the eastern counties is crowded, as we have described, with pebbles of chalk, which generally are not of local origin, but have come from north of the Wash. Whether from the bed of a river or from a sea-beach, they are certainly water-worn. But if preglacial, the supply would be quickly exhausted, so that they would usually be confined to the lower part of the clay. As it is, though perhaps they run larger here, they abound throughout. The so-called moraines near York (supposed to have been left by a glacier retreating up that vale), those in the neighborhood of Flamborough Head and of Sheringham (regarded as relics of the North Sea ice-sheet) do not, in my opinion, show any important difference in outline from ordinary hills of sands and gravels, and their materials are wholly unlike those of any indubitable moraines that I have either seen or studied in photographs. It may be said that the British glaciers passed over very different rocks from the Alpine; but the Swiss molasse ought to have supplied abundant sand, and the older interglacial gravels quantities of pebbles; yet the differences between the morainic materials on the flank of the Jura or near the town of Geneva and those close to the foot of the Alps are varietal rather than specific.

Some authorities, however, attribute such magnitude to the ice-sheets radiating from Scandinavia that they depict them, at the time of maximum extension, as not only traversing the North Sea bed and trespassing upon the coast of England, but also radiating southward to overwhelm Denmark and Holland, to invade northern Germany and Poland, to obliterate Hanover, Berlin and Warsaw, and to stop but little short of Dresden and Cracow, while burying Russia on the east to within no great distance of the Volga and on the south to the neighborhood of Kief. Their presence, however, so far as I can ascertain, is inferred from evidence[52] very similar to that which we have discussed in the British lowlands. That Scandinavia was at one time almost wholly buried beneath snow and ice is indubitable; it is equally so that at the outset the land stood above its present level, and that during the later stages of the glacial epoch parts, at any rate of southern Norway, had sunk down to a maximum depth of 800 feet. In Germany, however, erratics are scattered over its plain and stranded on the slopes of the Harz and Riesengebirge up to about 1,400 feet above sea-level. The glacial drifts of the lowlands sometimes contain dislodged masses of neighboring rocks like those at Cromer, and we read of other indications of ice action. I must, however, observe that since the glacial deposits of Möen, Warnemünde and Rügen often present not only close resemblances to those of our eastern counties but also very similar difficulties, it is not permissible to quote the one in support

[50] That this has occurred at Cromer is a very dubious hypothesis (see *Geol. Mag.*, 1905, pp. 397, 524). The curious relations of the drift and chalk in the islands of Möen and Rügen are sometimes supposed to prove the same action. Knowing both well, I have no hesitation in saying that the chalk there is, as a rule, as much in situ as it is in the Isle of Wight.

[51] About half-way across England and 810 feet above sea-level. P. F. Kendall, *Quart. Journ. Geol. Soc.*, LVIII., 1902, p. 498.

[52] A valuable summary of it is given in "The Great Ice Age," J. Geikie, ch. XXIX., XXX., 1894.

of the other, seeing that the origin of each is equally dubious. Given a sufficient "head" of ice in northern regions, it might be possible to transfer the remains of organisms from the bed of the Irish Sea to Moel Tryfaen, Macclesfield and Gloppa; but at the last-named, if not at the others, we must assume the existence of steadily alternating currents in the lakes in order to explain the corresponding bedding of the deposit. This, however, is not the only difficulty. The "Irish Sea glacier" is supposed to have been composed of streams from Ireland, southwest Scotland and the Lake District, of which the second furnished the dominant contingent; the first-named not producing any direct effect on the western coast of Great Britain, and the third being made to feel its inferiority and "shouldered in upon the mainland." But even if this ever happened, ought not the Welsh ice to have joined issue with the invaders a good many miles to the north of its own coast?[53] Welsh boulders at any rate are common near the summit of Moel Tryfaen, and I have no hesitation in saying that the pebbles of riebeckite-rock, far from rare in its drifts, come from Mynydd Mawr, hardly half a league to the east-southeast, and not from Ailsa Craig.[54]

As such frequent appeal is made to the superior volume of the ice-sheet which poured from the Northern Hills over the bed of the Irish Sea, I will compare in more detail the ice-producing capacities of the several districts. The present temperature of west central Scotland may be taken as 47°; its surface as averaging about 2,500 feet, rising occasionally to nearly 4,000 feet above sea-level. In the western part of the southern uplands the temperature is a degree higher, and the average for altitude at most not above 1,500 feet. In the Lake District and the northern Pennines the temperature is increased by another degree, and the heights are, for the one 1,800 feet with a maximum of 3,162 feet, for the other 1,200 feet and 2,892 feet. In north Wales the temperature is 50°, the average height perhaps 2,000 feet, and the culminating point 3,571 feet. For the purpose of comparing the ice-producing powers of these districts we may bring them to one temperature by adding 300 feet to the height for each degree below that of the Welsh region. This would raise the average elevation of central and southern Scotland to 3,400 feet and 2,100 feet, respectively; for the Lake District and northern Pennines to 2,100 feet and 1,500 feet. We may picture to ourselves what this would mean, if the snow-line were at the sea-level in north Wales, by imagining 8,000 feet added to its height and comparing it with the Alps. North Wales would then resemble a part of that chain which had an average height of about 10,000 feet above sea-level, and culminated in a peak of 11,571 feet; the Lake District would hardly differ from it; the northern Pennines would be like a range of about 9,000 feet, its highest peak being 11,192 feet. Southern Scotland would be much the same in average height

[53] From Moel Tryfaen to the nearest point of Scotland is well over a hundred miles, and it is a few less than this distance from Gloppa to the Lake District. In order to allow the Irish Sea ice-sheet to reach the top of Moel Tryfaen the glacier productive power of Snowdonia has been minimized (Wright, "Man and the Glacial Epoch," pp. 171, 172). But the difference between that and the Arenig region is not great enough to make the one incompetent to protect its own borderland while the other could send an ice-sheet which could almost cover the Clent Hills and reach the neighborhood of Birmingham. Anglesey also, if we suppose a slight elevation and a temperature of 29° at the sea-level, would become a center of ice-distribution and an advance guard to North Wales.

[54] The boulders of picrite near North Nobla, from Llanerchymedd, though they have traveled southward, have moved away much to the west.

as the first and second, and would rise, though rarely, to above 11,000 feet; the average in central Scotland would be about 11,400 feet, and the maximum about 13,000 feet. Thus, north Wales, the Lake District and the southern uplands would differ little in ice-productive power; while central Scotland would distinctly exceed them, but not more than the group around the Finsteraarhorn does that giving birth to the Rhone glacier. In one respect, however, all these districts would differ from the Alps —that, at 8,000 feet, the surface, instead of being furrowed with valleys, small and great, would be a gently shelving plateau, which would favor the formation of piedmont glaciers. Still, unless we assume the present distribution of rainfall to be completely altered (for which I do not know any reason), the relative magnitudes of the ice coming from these centers (whether separate glaciers or confluent sheets) could differ but little. Scotch ice would not appreciably "shoulder inland" that from the Lake District, nor would the Welsh ice be imprisoned within its own valleys.

During the last few years, however, the lake-hypothesis of Carvil Lewis has been revived under a rather different form by some English advocates of land-ice. For instance, the former presence of ice-dammed lakes is supposed to be indicated in the upper parts of the Cleveland Hills by certain overflow channels. I may be allowed to observe that, though this view is the outcome of much acute observation and reasoning,[55] it is wholly dependent upon the ice-barriers already mentioned, and that if they dissolve before the dry light of sceptical criticism, the lakes will "leave not a rack behind." I must also confess that to my eyes the so-called "overflow channels" much more closely resemble the remnants of ancient valley-systems, formed by only moderately rapid rivers, which have been isolated by the trespass of younger and more energetic streams, and they suggest that the main features of this picturesque upland were developed before rather than after the beginning of the glacial epoch. I think that even "Lake Pickering," though it has become an accepted fact with several geologists of high repute, can be more simply explained as a two-branched "valley of strike," formed on the Kimeridge clay, the eastern arm of which was beheaded, even in preglacial times, by the sea.[56] As to Lake Oxford,[57] I must confess myself still more sceptical. Some changes no doubt have occurred in later glacial and postglacial times; valleys have been here raised by deposit, there deepened sometimes by as much as 100 feet; the courses of lowland rivers may occasionally have been altered; but I doubt whether, since those times began, either ice-sheet or lake has ever concealed the site of that university city.

The submergence hypothesis assumes that, at the beginning of the glacial epoch, our islands stood rather above their present level, and during it gradually subsided, on the west to a greater extent than on the east, till at last the movement was reversed, and they returned nearly to their former position. During most of this time glaciers came down to the sea from the more mountainous islands, and in winter an ice-foot formed upon the shore. This, on becoming detached, carried away boulders, beach pebbles and finer detritus. Great quantities of the last also were swept by swollen streams into the estuaries and spread over the sea-bed by coast currents, settling down

[55] P. F. Kendall, *Quart. Journ. Geol. Soc.*, LVIII., 1902, 471.

[56] See for instance the courses of the Medway and the Beult over the Weald clay (C. Le Neve Foster and W. Topley, *Quart. Journ. Geol. Soc.*, XXI., 1865, p. 443).

[57] F. W. Harmer, *Quart. Journ. Geol. Soc.*, LXIII., 1907, p. 470.

especially in the quiet depths of submerged valleys. Shore-ice in Arctic regions, as Colonel H. W. Feilden[58] has described, can striate stones and even the rock beneath it, and is able, on a subsiding area, gradually to push boulders up to a higher level. In fact the state of the British region in those ages would not have been unlike that still existing near the coasts of the Barents and Kara Seas. Over the submerged region southward, and in some cases more or less eastward, currents would be prevalent; though changes of wind[59] would often affect the drift of the floating ice-rafts. But though the submergence hypothesis is obviously free from the serious difficulties which have been indicated in discussing the other one, it gives a simple explanation of the presence of marine organisms, and accords with what can be proved to have occurred in Norway, Weigatz Island, Novaia Zemlya, on the Lower St. Lawrence, in Grinnell Land and elsewhere,[60] it undoubtedly involves others. One of them—the absence of shore terraces, caves or other sea marks—is perhaps hardly so grave as it is often thought to be. It may be met by the remark that unless the glacial age lasted for a very long time and the movements were interrupted by well-marked pauses, we could not expect to find any such record. In regard also to another objection, the rather rare and sporadic occurrence of marine shells, the answer would be that, on the Norway coast, where the ice-worn rock has certainly been submerged, sea-shells are far from common and occur sporadically in the raised deltaic deposits of the fjords.[61]

An advocate of this view might also complain, not without justice, that, if he cited an inland terrace, it was promptly dismissed as the product of an ice-dammed lake, and his frequent instances of marine shells in stratified drifts were declared to have been transported from the sea by the lobe of an ice-sheet; even if they have been carried across the path of the Arenig ice, more than forty miles, as the crow flies, from the Irish Sea up the Valley of the Severn, or forced some 1,300 feet up Moel Tryfaen.[62] The difficulty in the latter case, he would observe is not met by saying the ice-sheet would be able to climb that hill "given there were a sufficient head behind it."[63] That ice can be driven uphill has long been known, but the existence of the "sufficient head" must be demonstrated, not assumed. There may be "no logical halting-place between an uplift of ten or twenty feet to surmount a *roche moutonnée* and an equally gradual elevation to the height of Moel Tryfaen," yet there is a common-sense limitation, even to a destructive *sorites*. The argument, in fact, is more specious than valid, till we are

[58] *Quart. Journ. Geol. Soc.*, XXXIV., 1878, p. 556.

[59] See p. 23, and for the currents now dominant consult Dr. H. Bassett in Professor Herdman's Report on the Lancashire Sea Fisheries, *Trans. Biol. Soc. Liverpool*, XXIV., 1910, p. 123.

[60] See "Ice Work," p. 221, and *Geol. Mag.*, 1900, p. 289.

[61] If, as seems probable, the temperature was changing rather rapidly the old fauna would be pauperized and the new one make its way but slowly into the British fjords.

[62] Critics of the submergence hypothesis seem to find a difficulty in admitting downward and upward movements, amounting sometimes to nearly 1,400 feet during Pleistocene ages; but in the northern part of America the upheaval, at any rate, has amounted to about 1,000 feet, while on the western coast, beneath the lofty summit of Mount St. Elias, marine shells of existing species have been obtained some 5,000 feet above sea-level. It is also admitted that in several places the preglacial surface of the land was much above its present level. On the Red River, whatever be the explanation, foraminifers, radiolarians and sponge spicules have been found at 700 feet above sea-level, and near Victoria, on the Saskatchewan, even up to about 1,900 feet.

[63] P. F. Kendall in Wright's "Man and the Glacial Period," p. 171.

told approximately how thick the northern ice must be to produce the requisite pressure, and whether such an accumulation would be possible. The advocates of land ice admit that, before it had covered more than a few leagues on its southward journey its thickness was less than 2,000 feet, and we are not entitled, as I have endeavored to show, to pile up ice indefinitely on either our British highlands or the adjacent sea-bed. The same reason also forbids us largely to augment the thickness of the latter by the snowfall on its surface, as happens to the Antarctic barrier ice. Even if the thickness of the ice-cap over the Dumfries and Kirkcudbright hills had been about 2,500 feet, that, with every allowance for viscosity, would hardly give us a head sufficient to force a layer of ice from the level of the sea-bed to a height of nearly 1,400 feet above it and at a distance of more than 100 miles.

Neither can we obtain much support from the instance in Spitzbergen, described by Professors Garwood and Gregory, where the Ivory Glacier, after crossing the bed of a valley, had transported marine shells and drift from the floor (little above sea-level) to a height of about 400 feet on the opposite slope. Here the valley was narrow, and the glacier had descended from an inland ice-reservoir, much of which was at least 2,800 feet above the sea, and rose occasionally more than a thousand feet higher.[64]

But other difficulties are far more grave. The thickness of the chalky boulder clay alone, as has been stated, not unfrequently exceeds 100 feet, and, though often much less, may have been reduced by denudation. This is an enormous amount to have been transported and distributed by floating ice. The materials also are not much more easily accounted for by this than by the other hypothesis. A continuous supply of well-worn chalk pebbles might indeed be kept up from a gradually rising or sinking beach, but it is difficult to see how, until the land had subsided for at least 200 feet, the chalky boulder clay could be deposited in some of the East Anglian valleys or on the Leicestershire hills. That depression, however, would seriously diminish the area of exposed chalk in Lincolnshire and Yorkshire, and the double of it would almost drown that rock. Again, the East Anglian boulder clay, as we have said, frequently abounds in fragments and finer detritus from the Kimeridge and Oxford clays. But a large part of their outcrop would disappear before the former submergence was completed. Yet the materials of the boulder clay, though changing as it is traced across the country, more especially from east to west, seem to vary little in a vertical direction. The instances, also, of the transportation of boulders and smaller stones to higher levels, sometimes large in amount, as in the transference of "brockram" from outcrops near the bed of the Eden valley to the level of Stainmoor Gap, seem to be too numerous to be readily explained by the uplifting action of shore-ice in a subsiding area. Such a process is possible, but we should anticipate it would be rather exceptional.

Submergence also readily accounts for the above-named sands and gravels, but not quite so easily for their occurrence at such very different levels. On the eastern side of England gravelly sands may be found beneath the chalky boulder clay from well below sea-level to three or four hundred feet above it. Again, since, on the submergence hypothesis, the lower boulder clay about the estuaries of the Dee and the Mersey must represent a deposit from piedmont ice in a shallow sea, the mid-glacial sand (sometimes not very clearly marked in this part) ought not to be more than

[64] *Quart. Journ. Geol. Soc.*, LIV., 1898, p. 205. Earlier observations of some upthrust of materials by a glacier are noted on p. 219.

forty or fifty feet above the present Ordnance datum. But at Manchester it reaches over 200 feet, while near Heywood it is at least 425 feet. In other words, the sands and gravels, presumably (often certainly) mid-glacial, mantle, like the upper boulder clay, over great irregularities of the surface, and are sometimes found, as already stated, up to more than 1,200 feet. Either of these deposits may have followed the sea-line upwards or downwards, but that explanation would almost compel us to suppose that the sand was deposited during the submergence and the upper clay during the emergence; so that, with the former material, the higher in position is the newer in time, and with the latter the reverse. We must not, however, forget that in the island of Rügen we find more than one example of a stratified gravelly sand between two beds of boulder clay (containing Scandinavian erratics) which present some resemblance to the boulder clays of eastern England, while certain glacial deposits at Warnemünde, on the Baltic coast, sometimes remind us of the Contorted Drift of Norfolk.

Towards the close of the glacial epoch, the deposition of the boulder clay ceased[65] and its denudation began. On the low plateaus of the eastern counties it is often succeeded by coarse gravels, largely composed of flint, more or less water-worn. These occasionally include small intercalations of boulder clay, have evidently been derived from it, and indicate movement by fairly strong currents. Similar gravels are found overlying the boulder clay in other parts of England, sometimes at greater heights above sea-level. Occasionally the two are intimately related. For instance, a pit on the broad, almost level, top of the Gogmagog Hills, about 200 feet above sea-level, and four miles south of Cambridge, shows a current-bedded sand and gravel, overlain by a boulder clay, obviously rearranged; while other pits in the immediate neighborhood expose varieties and mixtures of one or the other material. But, as true boulder clay occurs in the valley below, these gravels must have been deposited, and that by rather strong currents, on a hill-top—a thing which seems impossible under anything like the existing conditions; and, even if the lowland were buried beneath ice full 200 feet in thickness, which made the hill-top into the bed of a lake, it is difficult to understand how the waters of that could be in rapid motion. Rearranged boulder clays also occur on the slopes of valleys[66] which may be explained, with perhaps some of the curious sections near Sudbury, by the slipping of materials from a higher position. But at Old Oswestry gravels with indications of ice action are found at the foot of the hills almost 700 feet below those of Gloppa.

Often the plateau gravels are followed at a lower level by terrace gravels,[67] which descend towards the existing rivers, and suggest that valleys have been sometimes deepened, sometimes only reexcavated. The latter gravels are obviously deposited by rivers larger and stronger than those which now wind their way seawards, but it is difficult to explain the former gravels by any fluviatile action, whether the water from a melting ice-sheet ran over the land or into a lake, held up by some temporary barrier. But the sorting action of currents in a slowly shallowing sea would be quite competent to account for them, so they afford an indirect support to the hypothesis

[65] Probably deposits of a distinctly glacial origin (such as those near Hessle in Yorkshire) continued in the northern districts, but on these we need not linger.

[66] For instance, at Stanningfield in the valley of the Lark.

[67] These contain the instruments worked by paleolithic (Acheulean) man who, in this country at any rate, is later than the chalky boulder clay.

of submergence. It is, however, generally admitted that there have been oscillations both of level and of climate since any boulder clay was deposited in the district south of the Humber and the Ribble. The passing of the great ice age was not sudden, and glaciers may have lingered in our mountain regions when paleolithic man hunted the mammoth in the valley of the Thames, or frequented the caves of Devon and Mendip. But of these times of transition before written history became possible, and of sundry interesting topics connected with the ice age itself—of its cause, date and duration, whether it was persistent or interrupted by warmer episodes, and, if so, by what number, of how often it had already recurred in the history of the earth —I must, for obvious reasons, refrain from speaking, and content myself with having endeavored to place before you the facts of which, in my opinion, we must take account in reconstructing the physical geography of western Europe, and especially of our own country, during the age of ice.

Not unnaturally you will expect a decision in favor of one or the other litigant after this long summing up. But I can only say that, in regard to the British Isles, the difficulties in either hypothesis appear so great that, while I consider those in the "land-ice" hypothesis to be the more serious, I can not as yet declare the other one to be satisfactorily established, and think we shall be wiser in working on in the hope of clearing up some of the perplexities. I may add that, for these purposes, regions like the northern coasts of Russia and Siberia appear to me more promising than those in closer proximity to the north or south magnetic poles. This may seem a "lame and impotent conclusion" to so long a disquisition, but there are stages in the development of a scientific idea when the best service we can do it is by attempting to separate facts from fancies, by demanding that difficulties should be frankly faced instead of being severely ignored, by insisting that the giving of a name can not convert the imaginary into the real, and by remembering that if hypotheses yet on their trial are treated as axioms, the result will often bring disaster, like building a tower on a foundation of sand. To scrutinize, rather than to advocate any hypothesis, has been my aim throughout this address, and, if my efforts have been to some extent successful, I trust to be forgiven, though I may have trespassed on your patience and disappointed a legitimate expectation.

T. G. BONNEY

THE FERTILITY OF THE SOIL[1]

I BELIEVE it is customary for any one who has the honor of presiding over a section of the British Association to provide in his presidential address either a review of the current progress of his subject or an account of some large piece of investigation by which he himself has illuminated it. I wish I had anything of the latter kind which I could consider worthy to occupy your attention for the time at my disposal; and as to a review of the subject, I am not without hopes that the sectional meetings themselves will provide all that is necessary in the way of a general review of what is going forward in our department of science. I have, therefore, chosen instead to deal from an historic point of view with the opinions which have prevailed about one central fact, and I propose to set before you this morning an account of the ebb and flow of ideas as to the causes of the fertility of the soil, a question which has naturally occupied the attention

[1] Address by the chairman of the Agricultural Sub-section of the British Association for the Advancement of Science, Sheffield, 1910.

of every one who has exercised his reason upon matters connected with agriculture. The fertility of the soil is perhaps a vague title, but by it I intend to signify the greater or less power which a piece of land possesses of producing crops under cultivation, or, again, the causes which make one piece of land yield large crops when another piece alongside only yields small ones, differences which are so real that a farmer will pay three or even four pounds an acre rent for some land, whereas he will regard other as dear at ten shillings an acre.

If we go back to the seventeenth century, which we may take as the beginning of organized science, we shall find that men were concerned with two aspects of the question—how the plant itself gains its increase in size, and, secondly, what the soil does towards supplying the material constituting the plant. The first experiment we have recorded is that of Van Helmont, who placed 200 lb. of dried earth in a tub, and planted therein a willow tree weighing 5 lb. After five years the willow tree weighed 169 lb. 3 oz., whereas the soil when redried had lost but 2 oz., though the surface had been carefully protected meantime with a cover of tin. Van Helmont concluded that he had demonstrated a transformation of water into the material of the tree. Boyle repeated these experiments, growing pumpkins and cucumbers in weighed earth and obtaining similar results, except when his gardener lost the figures, an experience that has been repeated. Boyle also distilled his pumpkins, etc., and obtained therefrom various tars and oils, charcoal and ash, from which he concluded that a real transmutation had been effected, "that salt, spirit, earth, and even oil (though that be thought of all bodies the most opposite to water) may be produced out of water."

There were not, however, wanting among Boyle's contemporaries men who pointed out that spring water used for the growing plants in these experiments contained abundance of dissolved material, but in the then state of chemistry the discussion as to the origin of the carbonaceous material in the plant could only be verbal. Boyle himself does not appear to have given any consideration to the part played by the soil in the nutrition of plants, but among his contemporaries experiment was not lacking. Some instinct seems to have led them to regard niter as one of the sources of fertility, and we find that Sir Kenelm Digby, at Gresham College in 1660, at a meeting of the Society for Promoting Philosophical Knowledge by Experiment, in a lecture on the vegetation of plants, describes an experiment in which he watered young barley plants with a weak solution of niter and found how their growth was promoted thereby; and John Mayow, that brilliant Oxford man whose early death cost so much to the young science of chemistry, went even further, for, after discussing the growth of niter in soils, he pointed out that it must be this salt which feeds the plant, because none is to be extracted from soils in which plants are growing. So general has this association of niter with the fertility of soils become that in 1675 John Evelyn writes: "I firmly believe that where saltpeter can be obtained in plenty we should not need to find other composts to ameliorate our ground"; and Henshaw, of University College, one of the first members of the Royal Society, also writes about saltpeter: "I am convinced indeed that the salt which is found in vegetables and animals is but the niter which is so universally diffused through all the elements (and must therefore make the

chief ingredient in their nutriment, and by consequence all their generation), a little altered from its first complexion."

But these promising beginnings of the theory of plant nutrition came to no fruition; the Oxford movement in the seventeenth century was but the false dawn of science. At its close the human mind, which had looked out of doors for some relief from the fierce religious controversy with which it had been so long engrossed, turned indoors again and went to sleep for another century. Mayow's work was forgotten, and it was not until Priestly and Lavoisier, De Saussure, and others, about the beginning of the nineteenth century, arrived at a sound idea of what the air is and does that it became possible to build afresh a sound theory of the nutrition of the plant. At this time the attention of those who thought about the soil was chiefly fixed upon the humus. It was obvious that any rich soils, such as old gardens and the valuable alluvial lands, contained large quantities of organic matter, and it became somewhat natural to associate the excellence of these fat, unctuous soils with the organic matter they contained. It was recognized that the main part of a plant consisted of carbon, so that the deduction seemed obvious that the soils rich in carbon yielded those fatty, oily substances which we now call humus to the plant, and that their richness depended upon how much of such material they had at their disposal. But by about 1840 it had been definitely settled what the plant is composed of and whence it derives its nutriment—the carbon compounds which constitute nine tenths of the dry weight from the air, the nitrogen, and the ash from the soil. Little as he had contributed to the discovery, Liebig's brilliant expositions and the weight of his authority had driven this broad theory of plant nutrition home to men's minds; a science of agricultural chemistry had been founded, and such questions as the function of the soil with regard to the plant could be studied with some prospect of success. By this time also methods of analysis had been so far improved that some quantitative idea could be obtained as to what is present in soil and plant, and, naturally enough, the first theory to be framed was that the soil's fertility was determined by its content of those materials which are taken from it by the crop. As the supply of air from which the plant derives its carbonaceous substance is unlimited, the extent of growth would seem to depend upon the supply available of the other constituents which have to be provided by the soil. It was Daubeny, professor of botany and rural economy at Oxford, and the real founder of a science of agriculture in this country, who first pointed out the enormous difference between the amount of plant food in the soil and that taken out by the crop. In a paper published in the *Philosophical Transactions* in 1845, being the Bakerian Lecture for that year, Daubeny described a long series of experiments that he had carried out in the botanic garden, wherein he cultivated various plants, some grown continuously on the same plot and others in a rotation. Afterwards he compared the amount of plant food removed by the crops with that remaining in the soil. Daubeny obtained the results with which we are now familiar, that any normal soil contains the material for from fifty to a hundred field crops. If, then, the growth of the plant depends upon the amount of this material it can get from the soil, why is that growth so limited, and why should it be increased by the supply of manure, which only adds a trifle to the vast stores of plant food already in the soil? For example, a turnip

crop will only take away about 30 pounds per acre of phosphoric acid from a soil which may contain about 3,000 pounds an acre; yet, unless to the soil about 50 pounds of phosphoric acid in the shape of manure is added, hardly any turnips at all will be grown. Daubeny then arrived at the idea of a distinction between the active and dormant plant food in the soil. The chief stock of these materials, he concluded, was combined in the soil in some form that kept it from the plant, and only a small proportion from time to time became soluble and available for food. He took a further step and attempted to determine the proportion of the plant food which can be regarded as active. He argued that since plants only take in materials in a dissolved form, and as the great natural solvent is water percolating through the soil more or less charged with carbon dioxide, therefore in water charged with carbon dioxide he would find a solvent which would extract out of a soil just that material which can be regarded as active and available for the plant. In this way he attacked his botanic garden soils and compared the materials so dissolved with the amount taken away by his crops. The results, however, were inconclusive and did not hold out much hope that the fertility of the soil can be measured by the amount of available plant food so determined. Daubeny's paper was forgotten, but exactly the same line of argument was revived again about twenty years ago, and all over the world investigators began to try to measure the fertility of the soil by determining as "available" plant food the phosphoric acid and potash that could be extracted by some weak acid. A large number of different acids were tried, and although a dilute solution of citric acid is at present the most generally accepted solvent I am still of opinion that we shall come back to the water charged with carbon dioxide as the only solvent of its kind for which any justification can be found. Whatever solvent, however, is employed to extract from the soil its available plant food, the results fail to determine the fertility of the soil, because we are measuring but one of the factors in plant production, and that often a comparatively minor one. In fact, some investigators—Whitney and his colleagues in the American Department of Agriculture—have gone so far as to suppose that the actual amount of plant food in the soil is a matter of indifference. They argue that as a plant feeds upon the soil water, and as that soil water must be equally saturated with, say, phosphoric acid, whether the soil contains 1,000 or 3,000 pounds per acre of the comparatively insoluble calcium and iron salts of phosphoric acid which occur in the soil, the plant must be under equal conditions as regards phosphoric acid, whatever the soil in which it may be grown. This argument is, however, a little more suited to controversy than to real life; it is too fiercely logical for the things themselves and depends upon various assumptions holding rigorously, whereas we have more reason to believe that they are only imperfect approximations to the truth. Still this view does merit our careful attention, because it insists that the chief factor in plant production must be the supply of water to the plant, and that soils differ from one another far more in their ability to maintain a good supply of water than in the amount of plant food they contain. Even in a climate like our own, which the textbooks describe as "humid" and we are apt to call "wet," the magnitude of our crops is more often limited by want of water than by any other single factor. The same American investigators have more recently engrafted on to their theory another sup-

position, that the fertility of soil is often determined by excretions from the plants themselves, which thereby poison the land for a renewed growth of the same crop, though the toxin may be harmless to a different plant which follows it in the rotation. This theory had also been examined by Daubeny, and the arguments he advanced against it in 1845 are valid to this day. Schreiner has indeed isolated a number of organic substances from soils—dihydroxystearic acid and picoline-carboxylic acid were the first examples—which he claims to be the products of plant growth and toxic to the further growth of the same plants. The evidence of toxicity as determined by water-cultures requires, however, the greatest care in interpretation, and it is very doubtful how far it can be applied to soils with their great power of precipitating or otherwise putting out of action soluble substances with which they may be supplied. Moreover, there are as yet no data to show whether these so-called toxic substances are not normal products of bacterial action upon organic residues in the soil, and as such just as abundant in fertile soils rich in organic matter as in the supposed sterile soils from which they were extracted.

As then we have failed to base a theory of fertility on the plant food that we can trace in the soil by analysis let us come back to Mayow and Digby and consider again the niter in the soil, how it is formed and how renewed. Their views of the value of nitrates to the plant were justified when the systematic study of plant-nutrition began, and demonstrated that plants can only obtain their supply of the indispensable element nitrogen when it is presented in the form of a nitrate, but it was not until within the last thirty years that we obtained an idea as to how the niter came to be found. The oxidation of ammonia and other organic compounds of nitrogen to the state of nitrate was one of the first actions in the soil which was proved to be brought about by bacteria, and by the work of Schloesing and Müntz, Warington and Winogradsky we learned that in all cultivated soils two groups of bacteria exist which successively oxidize ammonia to nitrites and nitrates, in which latter state the nitrogen is available for the plant. These same investigators showed that the rate at which nitrification takes place is largely dependent upon operations under the control of the farmer: the more thorough the cultivation, the better the drainage and aeration, and the higher the temperature of the soil the more rapidly will the nitrates be produced. As it was then considered that the plant could only assimilate nitrogen in the form of nitrates, and as nitrogen is the prime element necessary to nutrition, it was then an easy step to regard the fertility of the soil as determined by the rate at which it would give rise to nitrates. Thus the bacteria of nitrification became regarded as a factor, and a very large factor, in fertility. This new view of the importance of the living organisms contained in the soil further explained the value of the surface soil, and demolished the fallacy which leads people instinctively to regard the good soil as lying deep and requiring to be brought to the surface by the labor of the cultivator. This confusion between mining and agriculture probably originated in the quasi-moral idea that the more work you do the better the result will be; but its application to practise with the aid of a steam plough in the days before bacteria were thought of ruined many of the clay soils of the Midlands for the next half century. Not only is the subsoil deficient in humus, which is the accumulated débris of previous applications of manure and

vegetation, but the humus is the home of the bacteria which have so much to do with fertility.

The discovery of nitrification was only the first step in the elucidation of many actions in the soil depending upon bacteria—for example, the fixation of nitrogen itself. A supply of combined nitrogen in some form or other is absolutely indispensable to plants and, in their turn, to animals; yet, though we live in contact with a vast reservoir of free nitrogen gas in the shape of the atmosphere, until comparatively recently we knew of no natural process except the lightning flash which would bring such nitrogen into combination. Plants take combined nitrogen from the soil, and either give it back again or pass it on to animals. The process, however, is only a cyclic one, and neither plants nor animals are able to bring in fresh material into the account. As the world must have started with all its nitrogen in the form of gas it was difficult to see how the initial stock of combined nitrogen could have arisen; for that reason many of the earlier investigators labored to demonstrate that plants themselves were capable of fixing and bringing into combination the free gas in the atmosphere. In this demonstration they failed, though they brought to light a number of facts which were impossible to explain and only became cleared up when, in 1886, Hellreigel and Wilfarth showed that certain bacteria, which exist upon the roots of leguminous plants, like clover and beans, are capable of drawing nitrogen from the atmosphere. Thus they not only feed the plant on which they live, but they actually enrich the soil for future crops by the nitrogen they leave behind in the roots and stubble of the leguminous crop. Long before this discovery experience had taught farmers the very special value of these leguminous crops; the Roman farmer was well aware of their enriching action, which is enshrined in the well-known words in the Georgics beginning, "Aut ibi flava seres," where Virgil says that the wheat grows best where before the bean, the slender vetch, or the bitter lupin had been most luxuriant. Since the discovery of the nitrogen-fixing organisms associated with leguminous plants other species have been found resident in the soil which are capable of gathering combined nitrogen without the assistance of any host plant, provided only they are supplied with carbonaceous material as a source of energy whereby to effect the combination of the nitrogen. To one of these organisms we may with some confidence attribute the accumulation of the vast stores of combined nitrogen contained in the black virgin soils of places like Manitoba and the Russian steppes. At Rothamsted we have found that the plot on the permanent wheat field which never receives any manure has been losing nitrogen at a rate which almost exactly represents the differences between the annual removal of the crop and the receipts of combined nitrogen in the rain. We can further postulate only a very small fixation of nitrogen to balance the other comparatively small losses in the drainage water or in the weeds that are removed; but on a neighboring plot which has been left waste for the last quarter of a century, so that the annual vegetation of grass and other herbage falls back to the soil, there has been an accumulation of nitrogen representing the annual fixation of nearly a hundred pounds per acre. The fixation has been possible by the azotobacter on this plot, because there alone does the soil receive a supply of carbohydrate, by the combustion in which the azotobacter obtained the energy necessary to bring the nitrogen

into combination. On the unmanured plot the crop is so largely removed that the little root and stubble remaining does not provide material for much fixation.

Though numerous attempts have been made to correlate the fertility of the soil with the numbers of this or that bacterium existing therein, no general success has been attained, because probably we measure a factor which is only on occasion the determining factor in the production of the crop. Meantime our sense of the complexity of the actions going on in the soil has been sharpened by the discovery of another factor, affecting in the first place the bacterial flora in the soil and, as a consequence, its fertility. Ever since the existence of bacteria has been recognized attempts have been made to obtain soils in a sterile condition, and observations have been from time to time recorded to the effect that soil which has been heated to the temperature of boiling water, in order to destroy any bacteria it may contain, had thereby gained greatly in fertility, as though some large addition of fertilizer had been made to it. Though these observations have been repeated in various times and places they were generally ignored, because of the difficulty of forming any explanation: a fact is not a fact until it fits into a theory. Not only is sterilization by heating thus effective, but other antiseptics, like chloroform and carbon bisulphide vapor, give rise to a similar result. For example, you will remember how the vineyards of Europe were devasted some thirty years ago by the attacks of phylloxera, and though in a general way the disease has been conquered by the introduction of a hardy American vine stock which resists the attack of the insect, in many of the finest vineyards the owners have feared to risk any possible change in the quality of the grape through the introduction of the new stock, and have resorted instead to a system of killing the parasite by injecting carbon bisulphide into the soil. An Alsatian vine-grower who had treated his vineyards by this method observed that an increase of crop followed the treatment even in cases where no attack of phylloxera was in question. Other observations of a similar character were also reported, and within the last five years the subject has received some considerable attention until the facts became established beyond question. Approximately the crop becomes doubled if the soil has been first heated to a temperature of 70° to 100° for two hours, while treatment for forty-eight hours with the vapor of toluene, chloroform, etc., followed by a complete volatilization of the antiseptic, brings about an increase of 30 per cent. or so. Moreover, when the material so grown is analyzed, the plants are found to have taken very much larger quantities of nitrogen and other plant foods from the treated soil; hence the increase of growth must be due to larger nutriment and not to mere stimulus. The explanation, however, remained in doubt until it has been recently cleared up by Drs. Russell and Hutchinson, working in the Rothamsted laboratory. In the first place, they found that the soil which had been put through the treatment was chemically characterized by an exceptional accumulation of ammonia, to an extent that would account for the increased fertility. At the same time it was found that the treatment did not effect complete sterilization of the soil, though it caused at the outset a great reduction in the numbers of bacteria present. This reduction was only temporary, for as soon as the soil was watered and left to itself the bacteria increased to a degree that is never attained under normal conditions. For example, one of the Rothamsted soils employed con-

tains normally about seven million bacteria per gram—a number which remains comparatively constant under ordinary conditions. Heating reduced the numbers to 400 per gram, but four days later they had risen to six million, after which they increased to over forty million per gram. When the soil was treated with toluene a similar variation in the number of bacteria was observed. The accumulation of ammonia in the treated soils was accounted for by this increase in the number of bacteria, because the two processes went on at about the same rate. Some rearrangements were effected also in the nature of the bacterial flora; for example, the group causing nitrification was eliminated, though no substantial change was effected in the distribution of the other types. The bacteria which remained were chiefly of the class which split up organic nitrogen compounds into ammonia, and as the nitrate-making organisms which normally transform ammonia in the soil as fast as it is produced has been killed off by the treatment, it was possible for the ammonia to accumulate. The question now remaining was, What had given this tremendous stimulus to the multiplication of the ammonia-making bacteria? and by various steps, which need not here be enumerated, the two investigators reached the conclusion that the cause was not to be sought in any stimulus supplied by the heating process, but that the normal soil contained some negative factor which limited the multiplication of the bacteria therein. Examination along these lines then showed that all soils contain unsuspected groups of large organisms of the protozoa class, which feed upon living bacteria. These are killed off by heating or treatment by antiseptics, and on their removal the bacteria, which partially escape the treatment and are now relieved from attack, increase to the enormous degree that we have specified. According to this theory the fertility of a soil containing a given store of nitrogen compounds is limited by the rate at which these nitrogen compounds can be converted into ammonia, which, in its turn, depends upon the number of bacteria present effecting the change, and these numbers are kept down by the larger organisms preying upon the bacteria. The larger organisms can be removed by suitable treatment, whereupon a new level of ammonia-production, and therefore of fertility, is rapidly attained. Curiously enough one of the most striking of the larger organisms is an amœba akin to the white corpuscles of the blood—the phagocytes, which, according to Metchnikoff's theory, preserve us from fever and inflammation by devouring such intrusive bacteria as find entrance in the blood. The two cases are, however, reversed: in the blood the bacteria are deadly, and the amœba therefore beneficial, whereas in the soil the bacteria are indispensable and the amœba become noxious beasts of prey.

Since the publication of these views of the functions of protozoa in the soil confirmatory evidence has been derived from various sources. For example, men who grow cucumbers, tomatoes and other plants under glass are accustomed to make up extremely rich soils for the intensive culture they practise, but, despite the enormous amount of manure they employ, they find it impossible to use the same soil for more than two years. Then they are compelled to introduce soil newly taken from a field and enriched with fresh manure. Several of these growers here have observed that a good baking of this used soil restores its value again; in fact, it becomes too rich and begins to supply the plant with an excessive amount of nitrogen. It has also been pointed out

that it was the custom of certain of the Bombay tribes to burn vegetable rubbish mixed as far as possible with the surface soil before sowing their crop, and the value of this practise in European agriculture, though forgotten, is still on record in the books on Roman agriculture. We can go back to the Georgics again, and there find an account of a method of heating the soil before sowing, which has only received its explanation within the last year, but which in some form or other has got to find its way back again into the routine of agriculture. Indeed, I am informed that one of the early mysteries, many of which we know to be bound up with the practises of agriculture, culminated in a process of firing the soil, preparatory to sowing the crop.

My time has run out, and I fear that the longer I go on the less you will feel that I am presenting you with any solution of the problem with which we set out—"What is the cause of the fertility of the soil?" evidently there is no simple solution; there is no single factor to which we can point as *the* cause; instead we have indicated a number of factors any one of which may at a given time become a limiting factor and determine the growth of the plant. All that science can do as yet is to ascertain the existence of these factors one by one and bring them successively under control; but, though we have been able to increase production in various directions, we are still far from being able to disentangle all the interacting forces whose resultant is represented by the crop.

One other point, I trust, my sketch may have suggested to you: when science, a child of barely a century's growth, comes to deal with a fundamental art like agriculture, which goes back to the dawn of the race, it should begin humbly by accepting and trying to interpret the long chain of tradition. It is unsafe for science to be dogmatic; the principles upon which it relies for its conclusions are often no more than first approximations to the truth, and the want of parallelism, which can be neglected in the laboratory, give rise to wide divergencies when produced into the regions of practise. The method of science is, after all, only an extension of experience. What I have endeavored to show in my discourse is the continuous thread which links the traditional practises of agriculture with the most modern developments of science.

A. D. HALL

THE INTERNATIONAL ESPERANTO CONGRESS

AMERICA has been the scene of many conventions and congresses of a more or less international character, at which delegates representing many diverse lands and nationalities have gathered to discuss subjects of common interest. At these congresses, those attending have been almost as diverse with respect to language as to nationality and the halls of the congress and the places of social gathering and entertainment in connection with it, have usually been filled with well-nigh as much confusion as the historic plain of Shinar. Of course, each of these congresses has had its one or more official languages, in which papers were presented and official business transacted; many of those present being unable to take part in or fully enjoy the proceedings, because of lack of sufficient knowledge of some or all of the languages so used—to say nothing of the embarrassment caused when groups of the delegates met casually outside the regular sessions and free intercourse was restricted, or altogether prohibited, by the barrier of language. How many of those attending, handicapped by the paucity of their linguistic attainments, have looked back upon such gatherings with more or less regret, feeling that they had lost much, yet knowing full well, that from lack of time or otherwise, the possibility of increasing their

stock of tongues was remote and that they could only expect, in similar gatherings in the future, a repetition of the same disappointing experience!

But America has seen a new thing and those who had the pleasure of attending the International Esperanto Congress in Washington during the week of August 14–20, 1910, have experienced a new and pleasant sensation. They have seen a gathering, international in scope, entirely free from the objectionable features enumerated; a gathering of persons from many nations, as diverse as possible in their national characteristics and tongues, but alike in the one respect, that they spoke the artificial auxiliary language, Esperanto, and consequently had a common medium by means of which to exchange ideas when they met, either socially or in convention assembled. The international character of the congress can not be questioned, there having been noted by the writer persons from not less than twenty-three different nations and countries—their varying natural languages numbering eleven. In addition, many widely-separated parts of the world not included in this enumeration, because peopled or controlled by the same race, were in evidence; as, for example, India, Malta and Ireland, whose delegates were Englishmen. Other countries still were represented by men of an alien race; as, Italy by a Frenchman and Peru by an American. The nation without a country also sent its salutations by a Yiddish-speaking member of the Hebrew race.

With most of these persons I had the pleasure of conversing in Esperanto, though often absolutely ignorant of their language; as, for examples, Russian, Spanish or Croatian. One very pleasant social evening was spent in the company of a small party, which included, besides English and Americans, a Spaniard, a Russian, several Frenchmen, a Pole, a German, a Mexican and a Portuguese. None of these gentlemen knew all the languages represented and some knew only their own, yet they conversed together easily and freely. The experience of participating in such cosmopolitan gatherings as this, and still being able to comprehend and to be comprehended at all times, was not only novel but extremely pleasant. The thoughts and emotions of these men of other climes and tongues, which had been before as a sealed book, were at last approachable *at first hand,* and my mental horizon seemed to broaden and the way to a new world-view lay invitingly open before me.

Besides representatives of Esperanto societies all over the world, there were accredited to the congress, officially, delegates from twelve governments and governmental departments, including besides European and American countries, Persia, China and Japan. In addition, four states of the union sent official delegates and the United States government was also represented by officials of the navy, war and interior departments. All of these representatives who addressed the congress (and most of them did so) spoke in Esperanto except in a very few instances, notably in the case of the Chinese delegate, who used his native tongue. These few addresses were the only ones requiring translation to be universally understood, and even then, only *one* translation—into Esperanto—was necessary.

The entire official business of the congress was conducted in the international language and no translations or explanations, other than those noted above, were required, nor will any Esperantist delegate need to await publication in his own tongue in order to know what took place. The usual sectional meetings incidental to such general conventions were held during the week, and the special interests of jurists, physicians, journalists, teachers, pacifists, engineers, physicists and many others, were considered in the common tongue—this fact assisting in making the meetings more enjoyable, and it is to be hoped, more fruitful, as it permitted a freer and fuller comprehension and discussion of the subjects presented, than was possible under the old methods.

This congress can safely be said to be the first international one ever held in America at which such things were possible, and it is an object lesson in the feasibility, the value

and the practicability of the international auxiliary language.

Although the first Esperanto Congress here, it is the sixth international congress of Esperanto—five having been held previously, the first at Boulogne in 1905 and the others annually thereafter at Geneva, Cambridge, Dresden and Barcelona, in the order named. The congress of 1911 will sit in Antwerp. The same results as to easy intercommunication between peoples of different tongues, described above in connection with the Washington Congress, are reported as having been attained at all the former congresses, and it seems fair to assume that this outcome of continued experiment upon a large scale raises the presumption, that Esperanto is in position to make good its claims as an international means of communication. Even if we take no account of the rapidly spreading Esperanto movement, nor of the testimony which is almost daily to hand regarding its ability to smooth the way of the scientist, the philosopher or the merchant, whose interest reaches out beyond the narrow borders of his own land, still the success of these annual Esperanto congresses, which can not be gainsaid, at least provides sufficient *prima facie* evidence touching the worth of the language, as to demand thoughtful and thorough investigation upon the part of those interested in international conferences of any kind, or in furthering international intercommunication of any description. J. D. HAILMAN

PITTSBURGH

SCIENTIFIC NOTES AND NEWS

AT a special Degree Congregation held at Sheffield University in connection with a visit of the British Association, honorary degrees were conferred as follows: Doctor of Science—Mr. W. Bateson, F.R.S., the Rev. Professor T. G. Bonney, F.R.S., Sir William Crookes, F.R.S., Mr. Francis Darwin, F.R.S., Professor T. W. Rhys Davids, Sir Archibald Geikie, F.R.S., Professor E. W. Hobson, F.R.S., Sir Oliver Lodge, F.R.S., Sir Norman Lockyer, F.R.S., Dr. H. A. Miers, F.R.S., Sir William Ramsay, Professor C. S. Sherrington, F.R.S., Sir J. J. Thomson, F.R.S. Doctor of Engineering—Sir Joseph Jonas, Sir W. H. White, F.R.S. Doctor of Metallurgy—Mr. J. E. Stead, F.R.S.

DR. E. SCHULTZE, professor of agricultural chemistry at the Zurich School of Technology, has been given an honorary doctorate by the University of Heidelberg.

M. URBAIN, professor of chemistry at Paris, has been elected a corresponding member of the Madrid Academy of Sciences.

AMONG the representatives appointed to attend the opening of the Mexican National University on September 22 are Professor F. W. Putnam and Roland B. Dixon, from Harvard University, and Professor Franz Boas, from Columbia University.

PROFESSOR JUNIUS HENDERSON and Instructor Wilfred W. Robbins, of the University of Colorado, have been engaged in investigation in New Mexico, being connected with an exploring party of the Archeological Institute of America. Professor Henderson has been studying the geology and Mr. Robbins the botany of the Cliff Dweller region.

A COLLECTION of minerals, containing 200 specimens, for every high school in the state of Colorado, will be one of the results of the work done this summer by the State Geological Survey under the direction of Professor Russell D. George, state geologist. He is supervising five parties which are studying and reporting on the clays and minerals in various parts of the state. A volume containing reports from two of the districts has already been issued.

Nature states that Mr. J. Hewitt, assistant for lower vertebrates in the Transvaal Museum, and formerly curator of the Sarawak Museum, has been appointed director of the Albany Museum, Grahamstown, South Africa, in succession to Dr. S. Schonland, who has resigned owing to pressure of other work. The herbarium is still under the care of Dr. Schonland.

M. EUGÈNE ROUCHÉ, member of the Paris Academy of Sciences, in the section of mathe-

matics, died on August 19, at the age of seventy years.

M. Wilm, honorary professor of chemistry at Lille, has died at the age of seventy-seven years.

The death has occurred at Helsingfors, at the age of seventy-six years, of Karl Gustav Estlander, professor of esthetics in the university of that city.

It is announced from Paris that Madame Curie has isolated pure radium. Up to this time radium has been known only in the form of salts.

The copyright of the Encyclopedia Britannica has been acquired by the University of Cambridge Press from the London *Times*, which began the preparation of the eleventh edition some seven years ago. It is expected that the complete work in twenty-eight volumes will be issued simultaneously within six months.

The next meeting of the International Commission for Scientific Aeronautics will be held in 1912 in Vienna.

An International Conference on Town Planning will be held in London from October 10 to 16, under the patronage of the king, and under the auspices of the Royal Institute of British Architects. The council of the Royal Academy of Arts has promised to lend its galleries "for the display of the notable designs and illustrations of town planning and remodelling which have been collected from all parts of the world."

At the meeting of the British Iron and Steel Institute, to be held at Buxton, from September 26 to 30, the following papers will be read: "On Electric Steel Refining," by D. F. Campbell (London); "On the Hanyang Iron and Steel Works," by G. Chamier (Hankow, China); "On Manganese in Cast Iron and the Volume Changes during Cooling," by H. I. Coe, B.Sc. (Birmingham); "On Sulphurous Acid as a Metallographic Etching Medium," by E. Colver-Glauert (Berlin) and S. Hilpert (Charlottenburg); "On the Theory of Hardening Carbon Steels," by C. A. Edwards (Manchester); "On the Influence of Silicon on Pure Cast Iron," by A. Hague, B.Sc. (Birmingham) and T. Turner, M.Sc. (Birmingham); "On the Preparation of Magnetic Oxides of Iron from Aqueous Solutions," by S. Hilpert (Charlottenburg); "On the Manufacture of Rolled 'H' Beams," by G. E. Moore (Loughborough); "On the Utilization of Electric Power in the Iron and Steel Industry," by J. Elink Schuurman (Baden, Switzerland); "On the Briquetting of Iron Ores," by C. de Schwarz (Liège); "On some Experiments on Fatigue of Metals," by J. H. Smith (Belfast).

Lectures will be delivered in the lecture hall of the Museum Building of the New York Botanical Garden, Bronx Park, on Saturday afternoons, at four o'clock, as follows:

September 17—"Orchids, Wild and Cultivated," by Mr. G. V. Nash.

September 24—"The Botanical Gardens of Europe," by Dr. W. A. Murrill.

October 1—"Some Floral and Scenic Features of Jamaica," by Dr. M. A. Howe.

October 8—"Carnivorous Plants," by Dr. H. M. Richards.

October 15—"Autumn Flowers," by Dr. N. L. Britton.

October 22—"Plant Diseases and their Control," by Mr. F. J. Seaver.

October 29—"Explorations in Santo Domingo," by Mr. Norman Taylor.

November 5—"The Flora of Switzerland," by Professor E. S. Burgess.

November 12—"Some Economic Plants of Mexico," by Dr. H. H. Rusby.

November 19—"Cuba: Its Flora and Plant Products," by Dr. N. L. Britton.

The last issue of the University of Colorado Studies, now in its seventh volume, contains the following articles: "Pre-Thalesian Philosophy," by Professor Melanchthon F. Libby; "Sex Differences and Variability in Color Perception," by Professor Vivian A. C. Henmon; "Ants of Northern Colorado," by Instructor Wilfred W. Robbins; "Northern Colorado Plant Communities," by Professor Francis Ramaley; "Flow of Water in Irrigation Ditches," by Professor Clement C. Williams.

The Oregon Academy of Sciences was incorporated last month and placed on a permanent basis. While it has been doing active work for about five years it has never taken steps for a permanent organization until this year. According to its constitution the objects of the academy are "to encourage scientific research and learning, to promote the diffusion of scientific knowledge among its members and throughout the state of Oregon and to aid in the discovery and development of the natural resources of the state." The officers are: J. D. Lee, president, Portland; W. N. Ferrin, first vice-president, Forest Grove; John F. Bovard, second vice-president, Eugene; H. S. Jackson, third vice-president, Corvallis; A. E. Yoder, treasurer, Portland; A. W. Miller, curator and librarian, Portland; Frank W. Power, secretary, Portland.

It is stated in *Nature* that during the past month sixteen research students have been at work at the Port Erin Biological Station. The oceanography course conducted by Professor Herdman, with Dr. Dakin and Dr. Roaf, during the first half of August was attended by eight, and consisted partly of lectures and laboratory work in the biological station and partly of work at sea. One day was spent in fish-trawling on board the Lancashire sea-fisheries steamer, and other occasions in plankton work and dredging from the *Ladybird*. The contemplated addition of a new research wing at the back of the present building has now been decided on, and the work will be commenced in a few days. This new building will provide an addition to the library and a large experimental-tank room and two smaller research rooms with large tanks for physiological and other experimental work on the ground floor, and a series of eight separate research rooms, each with two windows, on the upper floor. The whole will be completed in time for use during Easter vacation. The addition is made necessary by the increase in the number of students and research workers at the Port Erin Biological Station. A circular letter stating that £350 would be required to build the new wing was issued by Professor Herdman in May last, and since then the sum of about £250 has been raised.

More cement was made and used in the United States in 1909 than in any preceding year and the price per barrel was lower than ever before. The production in 1908 was 52,910,925 barrels valued at $44,477,653; the production in 1909 was 64,196,386 barrels, valued at $51,232,979. The increase was mainly in the output of Portland cement—62,508,461 barrels, valued at $50,510,385, as against 51,072,612 barrels in 1908, valued at $43,547,679. The output of natural and puzzolan cement formed only a small percentage of the total cement production. The average price of Portland cement per barrel in 1909 was less than 81 cents; the average price per barrel in 1908 was 85 cents. Portland cement cost $3 a barrel in 1880, but by reason of improvements in method of manufacture it can now be profitably sold for 80 cents a barrel. In 1909 there were 103 Portland cement plants in operation, an increase of 5 over the number working in 1908. Of these plants 21 were in Pennsylvania, 12 in Michigan, 10 in Kansas, 8 in Ohio, 7 in New York, 6 in Indiana, 5 in Illinois, and 5 in California. Most of this cement was used at home, for the United States has only a small export trade in cement, consuming from 1 to 3 per cent. of the production. This country's immense natural resources of cement-making materials and its many well-equipped cement plants, however, should make it a strong competitor for the outside world's cement trade.

The *Journal* of the Royal Society of Arts notes that the wide reaches of waste land in Singapore, which have been of no use since the culture of gambier, coffee and pepper was given up, are now the scenes of great activity. Rubber plants are being set up over these deserted wastes and seem to do well. In the suburbs of Singapore city a considerable area of swamp land has been drained and converted into a nursery for Para rubber plants, which are sold at a good profit to the planters on the island. In Malacca there were formerly many square miles of land, the hiding

place of the tiger and other big game, which have been transformed into fine rubber plantations, and now Malacca, which has for years been largely neglected, is in a flourishing condition. A short time ago there was no banking institution in the town of Malacca; to-day three banks are doing a good business, and the place is rapidly becoming an important center.

UNIVERSITY AND EDUCATIONAL NEWS

Jesse T. Bonney, of Norfolk, Va., leaves an estate of about $400,000, subject to the dower rights of his wife, to educational institutions for girls which he established. The widow's dower, which is one third of the whole estate for life, goes to the institutions after her death.

In May the Denver and Gross College of Medicine signed a contract by which it unites with the School of Medicine of the University of Colorado. The Denver and Gross College has discontinued the teaching of the first two years of the medical curriculum and on or before the first of January, 1911, will discontinue the teaching of the remaining years as well. A constitutional amendment permitting the university to conduct the last two years of the medical course in Denver will be submitted to the people of the state.

The following appointments have been made in the University of North Carolina for the coming session: Dr. Robert A. Hall, formerly assistant professor in Clemson College, associate professor of organic chemistry; Dr. James M. Bell, U. S. Bureau of Soils, associate professor of physical chemistry; Hampden Hill, instructor in analytical chemistry; Parker H. Daggett, of Harvard University, professor of electrical engineering; V. L. Chrisler, M.S. (Nebraska), assistant in physics in the University of Nebraska, instructor in physics; Guy R. Clements, instructor in Williams College, professor of mathematics; T. R. Eagles, professor of mathematics in Bethany College, West Virginia, instructor in mathematics. M. H. Stacy, formerly associate professor of civil engineering, has been promoted to professor of civil engineering and T. F. Hickerson has been advanced to associate professor of civil engineering.

Lawrence W. Cole, A.B. (Oklahoma), Ph.D. (Harvard), recently professor at the University of Oklahoma and instructor at Wellesley College and in the Harvard Summer School, has been appointed professor of psychology in the University of Colorado, to succeed Vivian A. C. Henmon, A.B. (Bethany), Ph.D. (Columbia), who has been called to the University of Wisconsin.

The Vienna correspondent of the *Journal* of the American Medical Association writes that there are at present vacant three important chairs for medical instruction, those of the deceased Schnabel and Zuckerkandl (ophthalmology and anatomy, respectively), and of von Strümpell (medicine), whose sudden resignation caused so much comment in all circles. The successors have been nominated already by the recommendations of the medical faculty of the university; and Professor Demmer, of Graz, will take over the eye clinic in October; it will be remembered that this place was refused by Hess on account of the insufficient endowment and little space in the old clinic whence so much original investigation had come. The chair of anatomy has been offered to Tandler, of Vienna, who will probably be appointed. The successor of Strümpell will be either Chvostek or Ortner, both Austrians and both in very good standing in medical circles.

DISCUSSION AND CORRESPONDENCE

THE TEACHING OF ELEMENTARY PHYSICS

To the Editor of Science: Physics teachers will, no doubt, read with considerable interest the discussions on the teaching of elementary physics which have been going on in Science. While I was not present at the Boston meeting nor on Professor Hall's mailing list, I should like to venture to comment upon his paper.

It seems to me that propositions 1, 2, 3 and 4 might very well be accepted, as well as first

four lines of No. 5. Proposition 6, also, is a good one. Propositions 7, 8 and 9 seem to me will eventually go by the board, as either unnecessary or wide of the mark. No. 8, for example, is an impossibility, as has been pointed out by Professor Magie.

I also find myself in agreement with those who would readily dispense with any high school physics for college students provided the student is mature, earnest and of general good training. It is not a question of having a previous knowledge of physics, but of capacity for plenty of hard work and of close application.

I am also inclined to sympathize with Professor Mann's position that the best judge of what a *high school* course in physics should be is the *high school* instructor himself. After all, is not the problem of high school physics one that the high school instructors should be allowed to work out independent of any overlordship on the part of the universities? There is, I believe, a justly growing resentment and impatience on the part of high school instructors at the dictation of the universities. The colleges and universities can well afford to let them work out their own four years' problem, asking only that such examples of their product as come up to the universities be creditable representatives of their labor. I am sure that the high school instructors are just as ambitious as the universities and colleges to show results, and I am inclined to believe that a good deal of the dictation on the part of higher education to the secondary schools handicaps instead of helps them. I am also inclined to believe that in letting the high school instructor have free scope with his high school course he should stop asking colleges and universities to give advanced credit to the high school students. It is for these reasons that it seems to me that propositions 7, 8, 9 and 6, also, are unnecessary, as well as the latter part of proposition 5.

The question of dynamics in section 9 is one which I hope the high schools would answer by teaching and not by omitting the subject of kinetics. It seems to me unfortunate for high school students to pass out into the world with no attempt at quantitative ideas in this subject, and that the high school teacher is likely to gain rather than lose by meeting the issue squarely instead of evading it merely because it is hard. While this is my view, I would be perfectly willing to leave the solution of this question, with all the rest of the high school course, to the high school teachers.

In closing I would express a hope, as does Dr. Hall, that the discussion may go on and not be closed even with his most excellent discussion. John C. Shedd

Olivet, Mich.

SCIENTIFIC BOOKS

Ancient Plants. By M. C. Stopes. Pp. viii + 198, figs. 122. London, Blackie & Son, Ltd. 1910.

This well-written and well-illustrated little book furnishes another striking illustration of the difficulty of writing in a non-technical way about a technical subject. As is usually the case, some aspects are made too primer-like while others are highly theoretical and out of place, as for example the concluding discussion in the present work regarding the probable future evolution of plants.

The work is well planned and the facts presented seem in general to bear close scrutiny, although many of the geological statements, while true for Great Britain or even western Europe, hardly apply to the rest of the world. The book is typically English, and will no doubt prove a very useful elementary text in that country. The author's frequent use of the phrase "microscopical standpoint" well serves to illustrate the point of view and explains her statement in the introduction that Williamson was the foremost contributor to paleobotany. No one will dispute Williamson's well-earned renown, but it is very doubtful if he would be considered the foremost contributor to even Carboniferous paleobotany outside of England, and his influence is more or less responsible for the neglect with which the splendid Tertiary floras of the south of England have been treated. Again Lindley & Hutton's "Fossil Flora of Great Britain" is

a classic, but like some other classics it has little except a historic interest at the present time and is not comparable to the contemporaneous work of Brongniart or Sternberg.

It is reassuring to see that British paleobotanists have readmitted ferns to the Paleozoic flora after having banished them almost completely a few years ago, and another praiseworthy feature of Miss Stopes's book is her recognition of the grave objections to the derivation of the angiosperms from the Mesozoic cycadophytes. To mention certain points which strike the reviewer as misleading, it is very doubtful if the older Paleozoic was of longer duration than the balance of time since its close. Again, Newer Mesozoic, Upper Mesozoic and Upper Cretaceous are used as synonyms, and if we are to understand that Lower Mesozoic includes Lower Cretaceous then monocotyledons and dicotyledons are well represented in the Lower Mesozoic of both Europe and America, despite the author's statement to the contrary.

The wide differences between floras of pre-Tertiary epochs are entirely fictitious, and it may be questioned if some of the Triassic "Equisetites" are not nearer the Paleozoic Calamites than they are to modern equisetums. It is true that *Neocalamites* and *Pseudannularia* are not petrified, but they are almost wholly unlike *Equisetum*. The differences between the Permian and Triassic floras has the sanction of long-continued reiteration, but that the statement is venerable does not make it true, and the more we know of the somewhat scant floras of the earlier Triassic the more Paleozoic their affinities appear.

If in an elementary work it is permissible to speak of *Lepidodendron* and its allies as if they were club-mosses it seems like straining at a gnat to insist that the Mesozoic *Bennettitales*, so-called, were not cycads. The differences between Cycadeoidea, to use the correct term, and modern cycads is scarcely greater than between *Lepidocarpon* and *Lycopodium*. Incidentally the author seems to have forgotten the rather numerous impressions of Paleozoic cycadophytes.

Miss Stopes's statement that fructifications are always the most important part of the plant will depend entirely upon the plant considered and the point of view. The established fact of the plasticity of the reproductive parts in most of the great Paleozoic plant phylæ is clear evidence that they furnish less reliable data for the determination of their points of contact with later plants than is furnished by stem anatomy or even foliar characters. A striking instance of a similar sort is furnished by the analogy between the so-called flowers of the Mesozoic cycadophytes and those of angiosperms.

Chapters VIII. to XVII., dealing with the past histories of plant families, are in the main well written, although that devoted to the angiosperms is relatively poor, as is usually the case in all discussions of this class of plants. The author's caution regarding the value of negative evidence in dealing with the Cretaceous flora seems to have been forgotten in her consideration of the very special kind of a flora which the Carboniferous rocks have almost universally yielded, and it may also be worth mentioning that other factors besides a cold season will account for leaf fall, and annual rings, so-called.

For those who have read thus far and have wondered what excuse the book has for its existence it may be pointed out that it is the only modern attempt at a summary of our present knowledge in the field of paleobotany. The author is quite at home in the realm of anatomy and morphology and gives a very readable summary of the present state of knowledge in this field which has been so admirably tilled of late years, particularly by the botanists of Great Britain. The chief defect of the book is the attempt to spread the morphology and anatomy of the Carboniferous swamp-flora, or the concepts derived from its study, over all geological time the world round.

It is perhaps unfair to expect an avowed primer to be a manual, nevertheless it remains true that a satisfactory paleobotanical text, either elementary or exhaustive, which will maintain a proper balance between fossils showing the external form of plants and those

revealing their internal structure still remains unwritten.

EDWARD W. BERRY
JOHNS HOPKINS UNIVERSITY

Fish Stories. By CHARLES FREDERICK HOLDER and DAVID STAR JORDAN. New York, Henry Holt and Co. American Nature Series.

A most readable book indeed is this by Holder and Jordan, interesting alike to the lover of angling, the lover of nature and the lover of good stories. A few short historical chapters, by way of introduction, put us in touch with the tellers of "fish stories" from Jonah down to John Hance, including such famous raconteurs as Pliny, Olaus Magnus, Sir John Mandeville and Izaak Walton, while a selection of the best of the classical yarns leaves the reader in a proper spirit of appreciation for the modern ones that follow.

But it must not be supposed that the book is entirely a record of prevarication. On the contrary, it contains much more of perfectly good natural history, told in such a manner that the unscientific reader can easily grasp it, yet losing nothing in scientific accuracy thereby—a rather unusual combination in nature books. The untruths which serve as a spicing for the work, are such "whoppers" that even the most guileless and credulous reader will have no difficulty in distinguishing them as fiction.

Instructive and entertaining chapters treat of the occurrence, life histories and habits of the various trouts and salmons, the seal, the deep-sea fishes, coral-reef fishes, etc. In discussing the flying fishes, the authors support the view that the propelling force comes from the movements of the tail just as the fish is leaving the water, and that the paired fins act after the manner of an aeroplane. The scientific world is by no means agreed upon this point, as the authors admit, and many good observers are equally as insistent that the fins are moved in flight so rapidly as to deceive the eye ordinarily.

There is much information on the larger fishes of the sea that will clear up the hazy notions of the uninitiated, and a chapter is well devoted to the sea-serpent. This classical animal, which has given rise to more misunderstanding and downright prevarication than perhaps any other animal, is shown to be, under certain circumstances, a figment of the imagination induced by over-indulgence in the favorite "bait" of fishermen. The other class of stories is shown to be due to the misconceptions of untrained observers upon obtaining a partial view of various marine animals. The great "oar fish" (*Regalecus*), a long ribbon-like form with a high frill-like dorsal fin, which reaches a length of at least 22 feet, and occurs in both the Atlantic and Pacific oceans, is no doubt largely to blame for these stories. The much smaller sea-snakes, and perhaps some other elongated forms may also be responsible in part.

While the authors give us the benefit of their experience in angling for various sorts of fishes, they at the same time protest strongly against the practise of "pot-hunters" among fishermen, who take large numbers for the sake of a record, and, being unable to make use of them, allow them to rot on the bank. "Trout-hogs, we call them, but in doing so we owe apologies to the relatively well-behaved swine."

We can not help wishing there were more such books treating authoritatively of other animals in this delightful manner, imparting so much reliable information and at the same time affording the reader so much pleasure.

R. C. O.

The Freshwater Aquarium and its Inhabitants. By OTTO EGGELING and FREDERICK EHRENBERG. New York, Henry Holt and Co.

Some idea of the popularity of the standing aquarium as an object of study and means of recreation is afforded by the number of recent books bearing on the subject. The reviewer is aware of something like a dozen such issued within the past decade. The most recent of these, and the one under discussion, is largely a compilation simplified for the beginner, and professes to be "a guide for the amateur aquarist."

There is some good advice to the beginner concerning the form, placing, bottom, planting

and stocking of the aquarium, and also on the feeding and care of its inmates. These portions of the book appear altogether too brief, however, and it would seem that Mr. Eggeling, with his long experience in these matters, might have given us more of the benefit of it. He has chosen instead to devote three fourths of the book (or to be exact, 280 of the 352 pages) to descriptions and figures of aquarium plants and animals.

The figures are generally excellent with only a few of the old stereotyped sort and nearly all of them are from good photographs. The descriptions apparently suffer from too great an attempt to popularize—at any rate they are loosely written and often fail to give enough diagnostic characters to distinguish a species from its relatives. The few sunfishes mentioned, for example, could hardly be identified among the many others which are found in our streams and ponds and which thrive equally well in aquaria. Such descriptions can have no particular use except to acquaint the reader with the general characters of the group rather than the individual kind.

The authors would have done well to submit their scientific names to the scrutiny of a specialist before publishing them, and thus have avoided the use of antiquated nomenclature. This is especially true of the fishes, where a cursory examination reveals nearly a score of scientific names no longer regarded as correct. A number of cases of mis-spelling occur among these names also—*e. g., Cotostomus* for *Catostomus, Rhinichtys* for *Rhinichthys, Amiurus* for *Ameiurus, Etheostoma cœrulea* for *E. cœruleum, Pomotis elongatis* for *P. elongatus*. The parasitic fungus *Saprolegnia* also appears as *Saprolegnies,* and the word "milt" as milk!

The invertebrates are very inadequately treated, only aquatic insects and snails receiving mention. The dragonflies are omitted entirely from the former, though they are among the most interesting of aquatic larvæ and are easily kept and reared. Neither is any mention made of the crayfishes or other fresh-water crustacea—an unfortunate omission.

To make amends for some of these deficiencies there is a considerable amount of interesting natural history matter on the habits of the various forms in the aquarium.

The publishers have seen fit to make the volume about twice as large and heavy as necessary by the use of thick glazed paper and wide margins. But in spite of its many faults the book will no doubt be of real service to many amateurs in this alluring field of study, and will be useful in creating interest in the home aquarium and its inhabitants.

R. C. O.

SCIENTIFIC JOURNALS AND ARTICLES

THE *Journal of Experimental Zoology* for July contains the following articles: E. Newton Harvey, "The Mechanism of Membrane Formation and other Early Changes in Developing Sea-urchins' Eggs as bearing on the Problem of Artificial Parthenogenesis," with two figures; William M. Wheeler, "The Effects of Parasitic and other kinds of Castration in Insects," with eight figures; A. M. Banta, "A Comparison of the Reactions of a Species of Surface Isopod with those of a Subterranean Species," Part II.; A. H. Estabrook, "Effect of Chemicals on Growth in Paramecium," with one figure; G. H. Parker, "Olfactory Reactions in Fishes."

OPINIONS RENDERED BY THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE[1]

THIS comprises a history of the commission; method to be followed in submitting cases for opinion; list of cooperating committees on nomenclature; personnel of the commission; references to places of publication of the International Code; opinions 1–25. The first five are republished from SCIENCE.[2] Twenty of the opinions are here published for the first time. As the brochure will have a rather restricted distribution, a résumé of these opinions is here presented. The intro-

[1] Smithsonian Institution, Washington, Publication No. 1938, July, 1910, 8vo, pp. 61.

[2] Vol. XXVI., October 18, 1907, pp. 522, 523.

ductory portion has already appeared in SCIENCE (issue for September 2, 1910).

Opinion 6 is to the effect that where a genus contained originally only two species, and no type was specified, and a second author later removed one of the species as the monotype of a new genus, the remaining species became necessarily the type of the original genus.

Opinion 7 is to the effect that "the expression 'n.g., n.sp.' used in the publication of a new genus for which no other species is otherwise designated as genotype is to be accepted as designation under Art. 30*a*."

Opinion 8 relates to the retention of *ii* or *i* in specific patronymic names, and the ruling is to the effect that *ii* is to be retained when so originally employed, in accordance with Art. 19, which is: "The original orthography of a name is to be preserved unless an error of transcription, a *lapsus calami*, or a typographical error is evident." This is also the rule of the original A. O. U. Code (Canon XXXVII.), but in the revised edition of this code it is provided that masculine specific patronymics in the genitive singular are always to end in a single *i*, unless the name originally terminated with *i*, when another *i* is to be added. This amended rule has been followed in the new edition of the Check-List.

Opinion 9 deals with the use of the name of a composite genus for a component part of it requiring a name, the decision being that under some circumstances it may be so used, but not under certain other circumstances.

Opinion 10 relates to the designation of genotypes for genera with identical limits, proposed without designation of type. The ruling in this case is that "any subsequent author may designate the genotypes, and if the types designated are not specifically identical, the two generic names may (other things being equal) be used for restricted genera containing the types in question."

Opinion 11 deals with the designation of genotypes by Latreille, 1810, in his "Table des genres avec l'indication de l'espèce qui leur sert de type," and decides that "from the evidence submitted no reason is apparent why Latreille's type designations should not stand as such."

Opinion 12 relates to a case of preoccupation of names, generic and specific, and is decided on the principle of priority.

Opinon 13 relates to the use of a pre-Linnæan "specific" name, untenable under the law of priority, the case being one of Catesby's names (1743), reprinted later (1771) by Edwards but not adopted by him. Under Opinion 5, the 1771 reprint of Catesby does not render his names available.

Opinion 14 takes up the question of *Etheostoma* Rafinesque, 1819. At first view this seems a complicated case, but it is easily resolvable under Art. 30*a*. In its principal features the case is nearly parallel with that of *Ixoreus* Bonaparte, and upholds the decision of the A. O. U. Committee regarding the genotype of that genus.

Opinion 15 relates to *Craspedacusta sowerbii* Lancaster, and is settled by application of the law of priority, which clearly covers the case. The opinion reaffirms previous rules respecting what constitutes publication and the absence of any right on the part of an author over his published names "not common to other writers." This case gave opportunity for one of the commissioners to recommend the rule adopted by some botanists to establish an exempt class, *nomina conservanda*.

Opinion 16 considers the status of prebinomial specific names (published prior to 1758) under Art. 30*d*. The gist of this opinion is: "In deciding whether a case of absolute tautonymy is present (under Art. 30*d*), the citation of a clear prebinomial specific name in synonymy is to be construed as complying with the demands of Art. 30*d*. Examples: *Equus caballus* (*Equus* cited in synonymy in the sense of 'the horse'), *Alca torda* (*Alca* cited in synonymy in the sense of 'the alca')."

In connection with this opinion a singular error is to be noted on pp. 33 and 38, where the type of *Charadrius* Linn. is given as "*C.*

africanus," as determined by Allen. On page 38, it is said: "The species *C. africanus*, accepted as genotype by Allen, is not one of the original species of 1758." As a matter of fact Allen designated *C. apricarius*, one of the original species, as the genotype of *Charadrius* and made no reference whatever to *C. africanus*. Apparently this error could have originated only through a clerical error in transcription, *africanus* being written in place of *apricarius*.[3]

Opinion 17 is to the effect that the genera in Weber's "Nomenclator entomologicus," 1795, "are to be accepted, in so far as they individually comply with the conditions of the code."

Opinion 18 makes *Coluber hydrus* Pallas the type of *Hydrus* Schneider, under the principle of tautonymy, and is further an "adjudication" of Art. 30*d*.

Opinion 19 is on *Plesiops* Oken, 1817, ex "Les Plésiops" Cuvier, 1817, *vs. Pharopteryx* Rüppell, 1828, a case partly zoological, partly nomenclatorial, and the decision is provisional. The discussion of the case and the rulings have, however, important bearings. *Plesiops* had originally no other basis than a diagnosis. The author of *Pharopterus* later affirmed its identity with *Plesiops*.

Opinion 20 is on the question "Shall the genera of Gronow, 1793, be accepted?" Gronow's nomenclature is binary but not binomial. "His generic names, therefore, correspond to the provisions of the Code, and are to be accepted as available under the Code."

Opinion 21 is on the question "Shall the genera of Klein, 1744, reprinted by Walbaum, 1792, be accepted?" As Walbaum did not accept "the genera of Klein, 1744, he did not thereby give to Klein's genera any nomenclatorial status, and Klein's genera do not therefore gain availability under the present code by reason of being quoted by Walbaum." The case is also covered by Opinion 5, published in SCIENCE (*l. c.*) in 1907. This decision bears on other nearly parallel cases not here cited.

Opinion 22 relates to *Ceraticthys vs. Cliola*. *Ceratichthys* Baird and Girard, 1853, being a monotypic genus, the single species originally referred to it is its type, although the diagnosis was later modified and the type transferred to a later genus *Cliola*.

Opinion 23, on "*Aspro vs. Cheilodipterus*, or *Ambassis*." *Aspro* was published by Lacépède in 1803 in inedited manuscript of Commerson; the name was not adopted by Lacépède, but his publication of it prevents the use of *Aspro* for a later genus (Cuv. and Val., 1828). By selecting as genotype the third of the five species named under it by Commerson (no genotype having been designated), *Aspro* would become a synonym of the earlier genus *Cheilodipterus*.

Opinion 24. "*Antennarius* Commerson, 1798, and Cuvier, 1817, *vs. Histrio* Fischer, 1813." *Antennarius* was published by Lacépède in the same way as *Aspro*, and is in common use from Cuvier, 1817, but unless *Antennarius* is tenable from Lacépède it would be superseded by *Histrio* Fischer, 1813. As *Antennarius* was given nomenclatorial status by its publication (though by another author), "it may therefore be accepted as a generic name dating from 1798."

Opinion 25. "*Damesiella* Tornquist, 1899, *vs. Damesella* Walcott, 1905." Both names are accepted under "Art. 36, Recommendations." It is stated in the "discussion": "The only paragraph now in the code under which the names *Damesiella* and *Damesella* can be judged is the one reading '8, [recommendation] *k*. Words formed by an arbitrary combination of letters.' Under this paragraph, *Damesiella* is not identical with *Damesella*." The two names were both proposed in honor of the same man, Dr. W. Dames! They are thus identical in origin and construction, except that an *i* is added in *Damesiella*, presumably for euphony.

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[3] Also on page 38, "*Cervus*" appears in the list of bird genera in place of *Corvus*, and elsewhere in this brochure are minor typographical errors, implying hasty proofreading, among them being errors of date, as 1802 for 1803 (p. 56), 1898 for 1798 (p. 57), etc.

SPECIAL ARTICLES

NOTES ON A LITTLE-KNOWN SPECIES OF SNAKE, CHIONACTIS OCCIPITALIS

Two specimens of a very rare and peculiar little snake, *Chionactis occipitalis* (Hallowell) were recently presented to the zoological department of Stanford University. The species is restricted in its distribution to parts of the Mojave and Colorado deserts. Only a few specimens have fallen into the hands of herpetologists and they appear to have been imperfectly preserved and hence not very well described.

One of the specimens was secured by Mr. F. L. Weed at Calexico, California, February 20, from about a foot beneath the surface in a sand dune. Mr. Weed states that the species occurs in the Imperial Valley wherever there are dunes not far from water, but that specimens are only occasionally seen. The other example was received January 21 from an unknown source. The Calexico specimen was in a solution of formalin and somewhat faded. The other had been dead but a short time, the brilliant and striking life colors being perfectly preserved. The scales of the body were smooth and glistened with a soft polish like fine lacquer. The body was rich creamy white in color, the dorsal surface being slightly tinged with olive, and crossed by numerous bands of an intense brownish black, each space between the bands having a large, oval, transverse spot of bright reddish orange. The head was greenish blue above with a median reddish orange stripe on the edges of the internasals and prefrontals. When placed in spirits the bright colors rapidly disappeared, the yellow tint faded from the light areas and the dark bands lost much of their intensity.

The preserved specimen has a crescentric, black spot on the parietal region of the head, the horns extending forward to the eyes. The spot encroaches on the posterior part of the frontal and occupies a corner of each supraocular and the greater part of the parietal plates. There are thirty-one transverse blackish bands on the dorsal surface of the body and a terminal spot on the tail. On the ventral surface beneath the tenth band from the head is a black spot, following which in regular succession are similar ones which gradually grow broader until they connect with the dorsal bands forming complete annuli. Posteriorly, fourteen bands completely encircle the body, all being more or less constricted laterally. On the dorsal surface the bands average somewhat broader than the space occupied by two scales; ventrally they cover from two to three and one-half gastrosteges, often being very irregular in outline or somewhat broken up into separate blotches. The oval, reddish orange spots, so characteristic of the living example, fade in the preservative to deep rose, then pale pink, and at last completely disappear. They are separated on both sides from the black bands by a space equal to the width of one scale, and they extend ventrally to within two scales of the gastrosteges. The Calexico specimen was apparently of the same general color in life. The black areas are less restricted on the body, the fifth band from the head forming a complete annulus. There are but twenty-nine spots and bands on the body, the tip of the tail being white. The color notes published by different authors[1] were evidently based on the evanescent hues of preserved material.

In superficial anatomical characters the two specimens agree very closely. They measure 372 mm. in length, including the tail, which is 59 mm. They are rather stocky, cylindrical in shape and very muscular. The head is about as large around as the anterior part of the body, the neck not being perceptibly constricted. The snout is somewhat spatulate, broad when viewed from above, pointed when seen from the side. The rostral plate is very large, twice as wide as high, projecting 1.5 mm. beyond the anterior part of the symphyseal.

[1] Cope, *Proceedings U. S. National Museum*, 1891, p. 605; Boulenger, "Catalogue Snakes British Museum," II., 1894, p. 266; Brown, *Proceedings Academy Sciences Philadelphia*, 1901, p. 68.

It is narrowly concave beneath and broadly convex above. The upper, posterior edge recurves between the internasals, imparting to the latter well-rounded anterior median borders. The internasals are bounded laterally by the nasals and posteriorly by the prefrontals. The frontal is hexagonal, somewhat longer than wide; the anterior angle obtuse, the posterior acute. It lies directly between the orbits, is bounded anteriorly by the prefrontals, laterally by the supraocular and posteriorly by the parietals. Each prefrontal touches the preocular, loreal and nasal ventrally. The supraocular is about twice as long as wide and makes a broad contact with the preocular and postocular. The parietal plates are longer than wide, the length being considerably more than that of the frontal. The nasal is single, pierced a little above the center. It is in contact with the rostral and loreal. The loreal is elongate, wedge-shaped, and in contact with the first and second supralabials. There are two postoculars, the upper twice as large as the lower. Temporals, one to two. Supralabials, seven on either side, the third and fourth of which are beneath the eye; fourth longest, the last which closely resembles the scales behind it, smallest. Of the seven infralabials the fourth is the largest. The first meets the corresponding one of the opposite side behind the symphyseal. The symphyseal is acutely pointed anteriorly, the tip fitting a corresponding concavity in the rostral. The anterior genials are broad and somewhat more than twice as long as the posterior ones. The gular scales are well developed, the dorsal scales smooth, in fifteen rows, smallest near the middle of the back. The gastrosteges number 174, the urosteges 44. The anal plate is divided. The pupil is large and round. The tongue is black, tipped with white.

The rarity of specimens of this snake in collections is apparently due both to its restricted distribution and to its habit of burrowing in the desert sands. Little is known of its food or its general habits. Although probably not nocturnal, it may spend most of its time hidden from sight, much as do the similar little snakes *Contia mitis* and *Diadophis amabilis*.

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A NEW VARIETY OF THE SUNFLOWER

THE northern sunflower (*Helianthus annuus lenticularis* or *H. lenticularis* Dougl.) is exceedingly abundant in Colorado and New Mexico, where I have seen many thousands, possibly millions. In all these, I have never seen a noteworthy variation in the color of the rays, until a few days ago my wife discovered a single plant of a most remarkable variety, growing along with the common form, within sight of our house in Boulder. This variety, for which I propose the name *coronatus*, may be described as follows: Leaves much darker green; petioles strongly purplish; heads in bud dark, the ends of the bracts dark purplish; disc dark, normal; rays a full orange (darker than the type), strongly suffused, especially about the middle, with bright chestnut red, the color more or less streaky, the basal 3 or 4 mm. yellow; beneath, the rays have the middle third or more of about the apical two-thirds red.

We have moved the plant to our garden, and hope to increase it by seed. It will make a fine addition to the series of horticultural sunflower varieties, and it is hoped an interesting subject for experiments in crossing. According to Shull[1] the sunflowers are self-sterile, so it will be necessary to cross the new variety with the normal one and afterwards extract the pure strain of the variety.

In the manner of discovery, this case recalls that of the Shirley poppy, but the poppy had lost a character, while the sunflower has gained one, or more precisely, appears to have a double dose of the anthocyan pigment which is present in normal plants. It will be interesting to enquire whether there is any doubling of the chromosomes, after the manner of *Œnothera gigas*, but it hardly seems likely that any cytological character will be visible, accompanying the increase of pigment.

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[1] *Botanical Gazette*, February, 1908, p. 104.